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COMMENTS FOR A SYMPOSIUM ON THE COMPARATIVE STUDY OF COMMUNIST --ETC(U)

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COMMENTS FOR A SYMPOSIUM ON THE  
COMPARATIVE STUDY OF COMMUNIST FOREIGN POLICIES\*

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As I understand it, <sup>↓</sup>the 1971 regional AAASS symposium for which the papers presented here<sup>†</sup> were originally prepared asked the authors to address themselves to three questions with regard to the comparative study of Communist foreign policies: (1) *why* should we compare; (2) *what* should we compare; and (3) *how* should we compare. The papers before us are long on "why" arguments, short on "what" recommendations, and tell us almost nothing about "how" to do it. That is perfectly consistent with the difficulty of the respective questions asked. Unfortunately, the future of the comparative Communist foreign policy enterprise depends on the soundness of an integrated, rather than a sequential, answer to those questions.

Roger Kanet's approach to the "why" question is unobjectionable, but also not terribly useful. To argue against the reasons he advances for developing Comparative Communist Foreign Policies as a field would be like arguing against Motherhood: it might help us broaden our understanding of the foreign policy process in general; improve our understanding of how Communist policies differ from and are similar to the policies of non-Communist states; and offer deeper insights

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\* Remarks delivered at a Symposium on the Comparative Study of Communist Foreign Policies, co-sponsored by the California Arms Control and Foreign Policy Seminar, the Southern California Seminar on Communist Systems, and *Studies in Comparative Communism*, May 14, 1974, Marina del Rey, California; to be published in *Studies in Comparative Communism*.

† Roger E. Kanet, "The Comparative Approach to the Study of Communist Foreign Policy"; Henry Krisch, "Comparing Communist Foreign Policies: Some Thoughts as to Purpose, Content, and Method"; David D. Finley, "Comparative Communist Foreign Policy: What Should We Compare, Why, and How."



than we might otherwise achieve into the foreign policies of particular Communist states. But while the virtues of Motherhood are unassailable in the abstract, it is necessary to arrive at puberty before getting pregnant, impregnation should be avoided when there is a high risk of miscarriage, and one ought to think about whether having a baby is the most useful or sensible *next* thing to do anyway. These caveats apply also to the enterprise advocated in the Kanet paper.

David Finley's discussion of "why" we should do comparative Communist foreign policy research, in my view, provides a much more useful point of departure, because he at least makes a start in the direction of distinguishing among objectives to be served by such comparative studies. The utility of the comparative approach depends in the first instance on what it is we need or want to find out. Having determined that, we can then explore what comparative advantages the comparative approach might offer regarding the things we most want to learn about, given the present state of the comparative politics art and the accumulated body of knowledge about the formulation and implementation of foreign policy in Communist-ruled states.

Finley invites us to assess the utility of the comparative approach to Communist foreign policies by asking whether it can improve our *ability* to make valid statements that are *policy-relevant*, *theory-relevant*, or both. His answer, as I read it, is that if comparison of Communist foreign policies permits us to achieve what comparison theoretically can achieve by organizing and interrogating data in a comparative way, then it will contribute positively to both theory-building and policy-relevant analysis. While I believe that Finley fails to provide credible justification for his optimism, his approach at least suggests that the comparative approach may be more useful and workable for some purposes than for others. I find his argument somewhat more persuasive on the theory-building than on the policy-relevant side.

Indeed, for the theory building enterprise, a comparative approach is indispensable. Comparison is at the very heart of scientific inquiry. For theory building, the "why" question virtually answers itself: because comparison is what it's all about. But *what*

to compare again depends on what kinds of theory one wants to build. In the international politics field, one can compare all actors, which is unwieldy for most purposes; the most powerful or influential actors, measured in a variety of quantifiable ways; or sub-systems of actors, defined according to common membership in an alliance system, geographical propinquity, similarities in size, population, language group, political system, and so forth. One can study aspects of the behavior of all of these sets of actors. One can also study foreign policies comparatively without comparing the policies of more than one national actor. One can, for example, study comparatively the foreign policies of, say, the Kennedy and Nixon Administrations in this country, or the Khrushchev and Brezhnev regimes in the Soviet Union. It is not clear to me that the sub-set "Communist foreign policies" taken as a whole is of such great interest for enough burning issues of comparative foreign policy studies to make a large investment in such a professional field an urgent matter of business. The authors make no effort to define that sub-set so I assume they are talking about the foreign policies of states that are ruled by Communist Parties, or at least by parties that claim derivation from a common Leninist source. That is a crisp enough criterion and I do not fault the authors for not trying to complicate it by Talmudic hair-splitting. The trouble is that the range of substantive foreign policies now embraced by units sharing that particular common denominator is so wide that the most interesting theoretical question seems to me to be: what difference does rule by parties that call themselves Communist make for the formulation and implementation of foreign policy by those national actors? And that question in turn requires comparison with non-Communist foreign policies.

On the other hand, there are theoretical issues that can be usefully explored by comparing sub-sets of the set of Communist-ruled states' foreign policies. If one is interested in exploring how subordinate members of an alliance system dominated by a hegemonal power attempt to assert distinctive national interests, the East European members of the WTO make a promising group for comparison.



At the very high level of generality and aggregation that comparative foreign policy theory building aims, I suppose it is possible that comparative Communist foreign policies, as a sub-field, can contribute something to moving the theory building enterprise closer to the goal of acquiring the capability to make statements about foreign policy that have universal validity. The trouble is that the kinds of statements that I see in the social science literature about international politics that can lay any claim to universal validity, I just don't find very useful for my own work, nor intrinsically very interesting. I doubt that incorporating the kinds of propositions about *Communist* foreign policies that can be validated into that body of statements will increase its utility or enlarge its substantive scope.

I hope that what I have said so far does not simply expose my biases, but also makes clear that my judgments about utility are conditioned by my own research interests. Of the two criteria proposed by Finley for judging the utility of comparative Communist foreign policy research, my own professional interest centers on its potential contribution to policy-relevant research. Here my expectations regarding payoff have to be quite modest. It's not at all that comparative work *per se* is irrelevant to policy research; it's just that with respect to what I believe needs most to be done to improve our analytical work on the foreign political behavior of Communist states, the kinds of comparative research that can be done and that are being proposed seem to me to have very little to offer.

Kanet puts his finger on the problem when he notes that all of the quantitatively-oriented comparative analyses of Communist foreign policies done so far have focused almost exclusively on foreign policy *outputs*, virtually ignoring the input side. The reason for this, as he points out, is that we have reasonably good information on some conventional policy outputs -- like UN voting -- and very little, especially not "hard" or quantifiable data, on inputs to the policy-making process in Communist countries. I agree with this, but not with the inference that Kanet draws from these facts about what should be done next.

Kanet concludes from this that in the foreseeable future, in comparative Communist foreign policy studies, as well as in country studies, it will continue to be profitable to focus on the analysis of policy outputs, utilizing such quantitative data as can readily be obtained -- he specifically mentions trade data and tourist and communications flows -- and developing time series for the kinds of quantitative output data that are available. From my perspective, this simply means leaping over the research issues of highest salience -- those that have to do with the foreign policymaking process and inputs to it -- and projecting into the comparative area, the substantively thin, often trivial, quantitative, output-oriented country studies that are at best only marginally relevant to policy issues.

Given the limited resources available for the study of Communist countries' foreign policies, this does not seem to me to be a sufficiently high value endeavor to warrant much priority. What needs most to be done in our field now is to improve our understanding of the foreign policy decisionmaking process in Communist states and of the most critical inputs to it. This is an indispensable prerequisite for developing a capability to make contingent predictions about foreign policy outputs under specified conditions, which is of interest both to theory and policy. We have barely scratched the surface here, even in our studies of the most important, the most consequential, and the most thoroughly researched of the Communist states, the USSR.

The authors of these papers are wrong, I think, to speak about the state of Communist foreign policy research as if there were a vast highly developed country literature standing behind it. Compared with the literature on both non-Communist foreign policy and on the domestic politics of Communist states, the Communist foreign policy field is grossly underdeveloped. This is particularly true of the case study literature, the building blocks which must provide the essential data base for purposes of theory building or other generalizations about the foreign policy behavior even of single states. There is nothing, for example, in the Soviet case study literature -- which is



incidentally very small -- to compare with the best of the case studies of U.S. foreign and military policy decisionmaking of recent years. The truth is that scholars of Soviet, not to speak of Chinese or other Communist country, foreign policies lack a sufficiently detailed or authoritative data base from which to write the most elementary, strictly journalistic account of governmental behavior on any major foreign policy issue or crisis comparable in verifiable accuracy to, say, what a team of *New York Times* investigative reporters are ordinarily able to piece together within weeks of a major U.S. foreign policy decision.

Because the data are so poor, few of us try to do anything comprehensive or detailed. We look for manageable research problems and the more manageable they are the less interesting, or, if you will, policy-relevant, they tend to be. The relatively few who do try -- for example, Slusser in his recent Berlin study<sup>\*</sup> -- are obliged to construct intricate pyramids of inferences derived from data that are several times removed from "hard" evidence. What is produced, often ingeniously, by such efforts, is seldom persuasive enough to command wide acceptance in the field, so that the storehouse of established knowledge from which new studies may draw grows very slowly and unevenly.

For all the talk we have heard about bringing Communist studies into the mainstream of social science, the Communist foreign policy field, on the plane of country studies, has barely been touched. *Techniques* of modern social science have been introduced into some studies, but on an ad hoc basis with little regard for a theoretical or conceptual framework. For example: the application of models and concepts derived from decision theory is perhaps the most promising current new development in the field of U.S. foreign policy studies. But almost nothing has been done to adapt, refine and operationalize

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<sup>\*</sup> Robert M. Slusser, *The Berlin Crisis of 1961*, Baltimore, Johns Hopkins University Press, 1973.

those concepts or models to the context of foreign policy decision-making in Communist countries.\* We should not expect to make dramatic improvements in Communist foreign policy studies simply by plugging in these models, which have been derived from the study of rather different decisionmaking systems on the basis of access to incomparably better and richer data than we have at our disposal. But still less can we expect to improve our performance significantly by leaping from our primitive country studies of Communist foreign policies to comparative studies on the same set of substantive issues that are bound to be still more primitive.

The most important immediate priority, in my view, is to improve and enlarge the established knowledge base about the context of foreign policy decisionmaking in Communist countries in ways that will render that knowledge more susceptible to disciplined inquiry, and to formulate and test theories in the middle range that are specific to what is known or what can be learned about the foreign policy decisionmaking contexts of the countries that are the targets of our investigation.

Now I believe that a comparative approach to the solution of some of the problems associated with these objectives could be useful. For example, comparisons that permit us to experiment with extrapolations in foreign policy functions from structurally similar cases that are relatively data rich to cases that are data poor might be helpful. These comparisons could be between foreign policymaking in different Communist countries, between Communist and non-Communist countries, or across different regimes or policy issues in a single country. The selection of the most useful comparison will depend on the problem to be worked.

Given the limited range, uneven quality, and generally underdeveloped state of our storehouse of knowledge about the foreign policies of Communist states, a carefully discriminating problem

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\* See Arnold L. Horelick, A. Ross Johnson and John D. Steinbruner, *The Study of Soviet Foreign Policy: A Review of Decision-Theory-Related Approaches*, Rand Report R-1334, The Rand Corporation, Santa Monica, California, December 1973.

oriented approach to the selection of research issues that can be best, or better, treated comparatively appeals to me more than a wholesale endorsement of comparative Communist foreign policy research that is justified on the assumption that there is bound to be useful fallout -- somehow -- from such an effort.